tonics which are best calculated to facilitate or hasten the nutritive processes, and, in this indirect way, to increase strength.

3. Lastly, it prescribes that some means shall be employed as occasion arises, to allay the abnormal nervous excitability, or the pain, for the sake of immediate relief, or to give that tranquility, so much needed, and which it is often impossible to obtain, except by artificial means.

Now, these are the prime indications clearly dictated by the pathological conditions exposed, and it so happens that they are exactly those gradually sanctioned by ages of the best practical experience in dealing with neuralgia, prior to any correct notions as to its pathology. But with this outline of views, I must close my somewhat hasty paper. You will observe that no claim of novelty is made in behalf of the views presented. They are presented simply because they are suggestive, and believed to have, if thought on, greater practical value than they have been supposed to possess.

ART. II.—PATHOLOGY AND TREATMENT OF REFLEX MOTOR SYMPTOMS—PARALYSIS, CONTRACTIONS, ETC.

A DISCOURSE BEFORE THE COUNTY MEDICAL SOCIETY OF NEW YORK, Jan. 8th, 1877.

BY EUGENE DUPUY, M. D.

(Member of the American Neurological Association; Corresponding Member, Paris Societe de Biologie, etc., etc.)

Paralyses from destruction of conductors only take place in the spinal cord. Yet even when conductors are diseased or destroyed in that organ, the palsy which is observed is mostly not due to the loss of function of the parts destroyed, but to an influence exercised by the diseased portion upon other areas of the cord. Provided a very few fibres are left healthy, the functions are accomplished. Paralysis depending upon destruction of the grey matter is very rare—only very little grey matter being necessary to perform all functions—not so, however, if considered as a consequence of alteration of the grey matter, which diseased grey matter exercises a paralytic influence upon other areas of the same tissue. Both conductors and cells can be destroyed to a very great extent, and yet no appreciable trouble in motion is detected. The nature of the lesion has little, if anything, in it, to differentiate kinds of reflex paralysis.

Reflex paralyses, therefore, are all the kinds of palsies which can be explained by the foregoing definition.

I consider all paralyses, in a word, to be reflex; some are due to inhibition, others to alteration of the blood vessels (functional), but reflex, nevertheless. In a word, I take it, that when there is a patch of sclerosis, or a small tumor, or a piece of bone protruding, and paralysis is observed, that symptom is to be ascribed to the irritative, inhibitory, or vascular irritation, to the influence exerted by that diseased portion of the cord upon other parts of the same.

For contractions, the same holds good, the difference between paralysis and contraction, appearing to me not to be due to a difference in the kind of irritation at work, but to the degree and to the idiosyncrasies of the patient, as there are numerous cases to prove that pressure by a protruding piece of bone, for example, in the same locality of the cord, and damaging it to the same extent, will cause in one case a palsy, in another a contraction, and oftentimes the one passes into the other, but more often the contraction becomes a palsy.

Some seventeen or eighteen years ago, Dr. Brown-Séquard undertook to give an explanation of these reflex palsies, based upon physiological results, obtained by experiments; he says, speaking of paraplegia, " that it is due to an excitation that has come to the spinal cord from a sensitive nerve. The excitation after having reached this nervous centre, may be reflected on the blood vessels of this very centre, or on those of the motor nerves or the muscles."

But since that time, not only has the theory of reflex paralysis given by Brown-Séquard, viz., contraction of spinal blood vessels, been questioned and criticised, but the existence even of such a kind of paralysis has been denied. Strange contradiction! for nobody ever doubted the existence of reflex contractions. I confess that Brown-Séquard's theory does not explain all the cases of palsy which he himself has recorded, but I believe we are not warranted to deny the existence of the disease. I will leave out older cases. It is the fashion now to reject cases recorded only a few years ago; let us comply.

First, I shall state that I include paralysis arising from brain disease, as well as from spinal disease, kidney disease, lung disease, etc., as reflex.

My learned friend, Dr. R. Lepine, of Paris, has recorded several cases of hemiplegia, occuring in persons suffering from pneumonia. At the autopsy there was no brain lesion to account for the motor symptoms.

Prof. Leudet has reported cases of empyema, in which, after the performance of the operation, for the withdrawal of pus, every time that a solution of carbolic acid was injected, paralysis of the arm on the corresponding side ensued.

Dr. Nicaise, one of the hospital surgeons of Paris, has reported two cases of strangulated hernia in the female, in which cases there was hemiplegia, complete, which, in one case, disappeared immediately after the operation of kelotomy and in the other case shortly after. In this last, subsequent autopsy disclosed no brain lesion.

Dr. Luys, of Paris, well-known for his ingenious but imaginative views on the physiology of the brain, has reported a case of aphasia in which the faculty of speech returned of a sudden. The Doctor has shown the brain of the patient to the Sociéte de Biologie. It was an example, he stated, of hypertrophy of the right third frontal convolution, consequent upon destruction of the left one, because it took up the function of the left one. This view, of course, is not tenable, because if it were as Dr. Luys states, the aphasia would have disappeared by degrees, and not all at once.

Dr. Proust has recorded another case of aphasia, in which there was a splinter of bone pressing on the dura-mater, far from the would-be seat of the faculty of speech, which, on being removed, allowed the patient to recover immediately, before the dressing of the wound had been completed. There are very numerous cases of the nature of those quoted above; but these have been reported by their authors very lately, and since the new ideas about localization of functions has come to light, and are, therefore, not thrown out by the law of fashion which will reject all old cases, although well observed and recorded. There are numerous cases of lesions of cutaneous nerves which have caused contractions, more or less permanent. A very localized patch of meningitis in some instances, worms in the intestines in others. Prof. Verneuil and Dr. Berger have lately reported, *de novo*, numerous cases of contractures of one or both wrists, consequent upon hernia. Cases of pressure on the dura-mater, spinal or cranial, with contractions following, are very numerous. But it appears that irritations of the nerve-endings are more prolific causes of contractions. I have known a case of lock-jaw which lasted for two days, and which depended solely upon this, that a piece of exploded shell had lodged in the neighborhood of the scrotum and the inguinal fold, having hardly lacerated the subjacent tissues; it was during the French war, and as soon as the man could be taken care of, the foreign body was removed and the tetanus subsided. Another case in which there was a very superficial wound, in fact, a mere tearing of the skin of the inner upper part of the thigh, which, during the granulation period, offered a fine opportunity of testing the reflex theory. I discovered that every time that a camel-hair brush, loaded with carbolized oil, was being passed upon a certain granulation area, the patient at once became rigid for some time; that granulation area was cut away by means of a pair of curved scissors, and no more rigidity was observed afterwards.

All these cases, and many others which everybody can find in medical papers, permit one to say, therefore, that paralyses as well as contractions of a reflex character do exist.

The theory offered by Brown-Séquard, to explain them, was first opposed by Dr. (now Sir William) Gull and Dr. Pavy, by Weir Mitchell and by Jaccoud, and by many others.

Sir Wm. Gull and Dr. Pavy not only analyzed some cases so as to show that there were in them some organic lesions, but undertook a series of experimentations upon animals to refute Brown-Séquard's affirmations. Brown-Séquard had written that, while tying the hilus of a kidney, or lacerating or cutting away one supra-renal capsule, he had seen the blood vessels of the spinal cord contract. His two opponents declared that not only did they never succeed in obtaining that result, but even that the blood vessels of the spinal cord are not to be seen. The experiments of Sir William Gull and Dr. Pavy must have been carried on in a very unexpert manner. They do not state how long they have been in opening the spinal canal, nor the amount of blood lost by the animal, and in what dietetic state it was at the time of operation, which several factors being considered, we can account for the results of a negative character which they have obtained. I have several times repeated Brown-Séquard's experiment, and I have always had the same results that he has reported. Prof. Vulpian has also seen that faradization of a communicating nerve of a dog under the influence of woorara, will bring on a contraction of the blood vessels in the corresponding side of the spinal cord.

Dr. Brown-Séquard has also seen, what everybody can see by performing the same experiment, that punctures with a needle through the olivary column of the medulla oblongata, at once produce a spasm of many muscles, which may remain contracted for hours, days and weeks. So much for contractions.

But objections have been taken upon other grounds also, by Drs. Gull and Pavy. Sir W. Gull has made this remark, that it is astonishing that an abscess of the kidneys will cause no reflex paralysis, whereas a single irritation of the organ, as supposed by Dr. Brown-Séquard, will produce it. There is here a strange forgetfulness of a well-known physiological law, by one of the most distinguished British physicians. When the kidney is the seat of disorganization—abscess—by pus, the nerve-ends of the kidney parenchyma are destroyed, and it is well known that they have a hundredfold more power than nerve-trunks to arouse actions, normal or morbid, of the centres. I cannot refrain from giving a very striking instance. In those celebrated experiments on the guinea pigs, which are rendered artificially epileptic, it is well known that a zone exists covering the area comprised between the anterior angle of the eye to the shoulder; hence to the median line and the base of the ear, back to the posterior angle of the eye, which, on being simply titillated, brings on an attack of epilepsy. Now, if a section of the skin be made so as to uncover the trunk of one of the nerves, or successively all the nerves which animate that area of skin, it is found that no amount of titillation, or of squeezing even, will bring on anything else but pain. The same experiment may be repeated upon any number of epileptic animals, and will give identical results.

Weir Mitchell and Dr. Jaccoud have endorsed the criticism of Dr. Gull and Pavy, and say that the paralysis under consideration is due to exhaustion of nerve cells though work suddenly increased. They mean that if a sensitive cell receives too strong impressions it loses the faculty of registering any. They suppose, therefore, that in reflex paraplegia, for instance, the irritation from the kidneys which are the diseased organs producing the symptoms, being constant and of a more intense character, that area of the spinal cord in relation with the sensitive nerves of the kidney is exhausted, hence the paresis. It may be that such is the case in some instances, but I doubt it. If a nerve fibre is overstrained in the manner supposed by Weir Mitchell and by Jaccoud, the loss of function which ensues is seen to depend not upon exhaustion of the centre to which that nerve goes, but to the nutritive alteration, of that nerve itself, at the point irritated. This is a commonplace fact, in experimental physiology, and unless, therefore, it is proved that there is a necessity for the spinal cord to receive normal impressions from the kidneys in order to fulfill its functions with regard to the motor apparatus, I do not see the value of the argument; but, moreover, supposing things take place as Weir Mitchell and Jaccoud suggest, then it would be necessary also to admit that that area of grey matter of the spinal cord which receives the renal nerves is the functional centre for the innervation of the muscles of the legs? But even granting that point, since it is a generally received opinion that there are direct fibres from the bulb to the different parts of the nerve-centres, how is it that voluntary motion is also abolished, whereas if the theory were true only involuntary reflex movements ought to be abolished?

Handfield Jones has accepted both opinions, Brown-Séquard's contraction, and the 'inhibitory influence theory,' which he thinks explains the greater number of facts.

In Germany a certain number of experiments have been undertaken with the view of solving that disputed point.

Tiesler irritated the sciatic nerve of dogs and rabbits with substances likely to induce in it an inflammatory state. In the case of a rabbit he obtained paralysis which lasted for three days, when, the animal being killed, he discovered that although the central extremity of the nerve was not diseased except at the point irritated, yet there was an abscess at this point of the spinal cord, at which the nerve makes its exit from it.

Feinberg has also stated that he has observed softening of the cord after cauterisation of the sciatic nerve with caustic potash. He says that there was neither alteration of the central portion of the nerve nor inflammation ascending towards the spinal canal.

Leyden has based upon these facts his opinion that a great number of reflex palsies can be thus satisfactorily explained.

I have repeated these experiments of Tiesler and of Feinberg several times, but I have failed to obtain the results which they have reported. I am unwilling, however, to say that their results are only exceptional, because pathological science now possesses many observations carefully recorded, which show that a nerve in contact with a cancerous tumor, does present at times deposits of cancerous matter along its length, in several different portions, although the intermediate portions may be perfectly healthy. The results of the two above named authors, therefore, can be accounted for in the same way; migration of diseased matter through the lymphatic channels of the nerves.

Dr. Brown-Séquard has himself changed his views on the subject within the last eight years. He admits now that there are at least three modes of production of reflex paralysis. Reflex paralysis can be brought about by a reflex contraction of the spinal bloodvessels, by an inhibitory influence starting from a peripheral sensitive nerve end, and by ascending neuritis, or other degrees of alteration of nerves.

I believe that these three modes of production of paralysis do give an explanation, satisfactory enough for all known kinds of reflex palsies, including of course palsy from brain disease. There are certain collections of nerve matter in the brain which seem to have a special endowment to produce palsies of that nature. There is for instance that very singular and characteristic hemianesthesia and hemiplegia, occurring after a destruction of the internal capsule, and which can be so complete that even the special senses are altered unilaterally. There are good grounds for believing that those disorders are not consequent upon destruction of a set of conductors nor of a centre, but are due to an inhibitory influence.

Prof. Vulpian, Prof. Charcot, and Dr. Burq, have recently shown, that faradic electricity and other modes of electricity have the power not only to cure for a day or two after each application, the motor and sensitive palsy, but also to restore sight, hearing and taste, to their former degree of perfection.

Now, on the other hand, if we take into consideration the saying of Dr. Hughlings Jackson, that motor paralysis can exist and disappear although the destruction of tissue is unrepaired in the brain, provided only a small portion of the motor tract is diseased, we will find that we must very certainly admit that these symptoms while producing entire paralysis of motion and sensation, do not recognize the *loss of tissue* as their cause, for, during the existence as well as after the cure of the motor symptoms, the same destruction of nerve matter exists in the brain. Therefore that destruction of nerve matter is not *per se*, immediately the cause of the palsy, but it exerts an influence on some other parts to cause the palsies; the morbid state of the parts diseased endowing them with that faculty. Moreover, the therapeutic results of Prof. Vulpian, Charcot, Gubler, Granett, and those more remarkable and much older ones of Dr. Burq, have shown that the same patient thus affected with hemiplegia and hemianesthesia—unilateral—can be cured for some hours and days, and then again become affected—all the time the same *organic lesion* being present in the brain, and being in the *same condition* apparently; as some of the patients experimented upon had symptoms of hemichorea, which is well known to depend upon secondary affections of nerve fibres.

It is plain that the size of the lesion has no claim for consideration in these cases as Dr. Hughlings Jackson has stated—cases are numberless to prove that the destruction of a whole corpus striatum gives rise to no more complete paralysis, no more extensive, than lesion of only one of its ganglia, and *vice-versa*, etc.

If we leave out the cases of paraplegia occurring during the course of genito-urinary diseases, in which lesions of the sacral nerves were observed, or in which the muscles were diseased by rheumatism, etc., and which are, properly speaking, not reflex but peripheral paralyses, we will find that in every case of palsy, always some grey matter or ganglionic structures are found in the nervous arch. The law of Rouget is well known, that every time that we observe a reflex inhibitory action, we always discover a nerve cell on the tract of the nerve irritated. If we examine the pathology of contractures, we will find the same arguments equally serviceable. Every physician has seen cases of different degrees of contraction, due to so-called hysteria and organic lesions, give way for some time, or altogether, under the influence of the electrical fluid used in different modes, or of a fright or of a reflex action of some kind. Dr. Sayre has lately recorded some very interesting cases of contractures in young children, which gave way entirely after the operation for phimosis. In some cases operated on by Dr. Sayre, the relief has not been of long duration, if I am to judge from what I have seen, as some of those young patients have been under my care since the operation performed by Dr. Sayre, and are as yet suffering as much as before from the same troubles. I ought to say here *en passant* that these cases are not cases of reflex paralysis—as I have seen some similar ones recorded in different journals—but of reflex contractions; there is no paralysis of any muscle in the cases which I have seen. Those contractures following or dependent upon phimosis, or other disorders of the peins or clitoris in children from infancy up to the age of thirteen or more, are very peculiar—they affect identical groups of muscles in all the patients which I have seen; the extensors of the inferior limbs and the adductors, to such an extent that the limbs are crossed. They affect the deltoid, the biceps, the long supinators, and the flexors of the palm of the hand. Also to some degree the internal portion of the sterno-cleido-mastoid muscles. The two internal recti of the eyes, also, so that the patients have regular squinting, or often nystagmus since the muscles contract or relax continually according as the head is turned either side. In one case the tongue is in a state of tonic contraction. In another, it is only so when the patient tries to speak—in all there is an hypersecretion of saliva—which last symptom is very characteristic of the nature of the trouble, if the experiments of Cl. Bernard on the chorda tympani are kept in mind.

I have satisfied myself that this state does arise from increased tonicity of some muscles on the one hand, and of diminished tonicity on the other. The contractions are never very marked, nor do they occupy all the muscles just named, except when voluntary movement is attempted—it is chorea of some sort. But I have stated that palsies from brain diseases, as well as from spinal diseases, are always reflex phenomena, not due to destruction of centres or of conductors. I have given some proofs already of that opinion. I shall now consider aphasia, and show that it is also an inhibition or reflex paralysis of the speech centre, if there is such a centre. Trousseau has recorded several very interesting cases of aphasia, in which the patients, unable to say anything voluntarily, all at once the speech faculty being awakened by a reflex process, said one or more words which they were unable to say again when asked—as if a spring had been at work. My learned friend, Dr. Onimus, of Paris, in a most interesting paper on the subject, has even tried to establish that there is a phonomotor apparatus in the brain. Whether such a centre exists or not,—his arguments do not prove either very conclusively—one thing comes out clearly. He reports the case of a janitor who could say some words during recovery from hemorrhage in the brain. When the first syllables were uttered, he would readily complete the word; for instance, he was asked to name a plaster of Paris statuette near at hand; the doctor said *sta*, the first syllable of statue, but obtained no response. Whereat the wife of the patient who was at hand said *est*atue, when the sick man repeated it at once. Dr. Onimus then again said *est*, and the patient thereupon repeated readily, an *estatue*. This one case is very remarkable, as it shows that although the patient understood in either case, yet the prompting, or rather suggestion, had no effect until the mode of pronouncing used by uneducated people was adopted.

The cases of aphasia, for instance, which depend upon snake bites, as I have the report of some cases in the bulletin of the Societe d' Anthropologie, cannot be anything but reflex, unless we take it for granted that the snake poison has an elective action for the posterior portion of the left third frontal convolution,—where it is pretended by most that a speech centre is localized—which I am afraid does not appear much less than absurd.

These cases, together with cases of amaurosis following trigeminal neuralgia, can be explained by the celebrated experiment of Magendie, which shows that section of the trigeminal nerve does induce amaurosis. It also induces anosmia and loss of taste. Certainly there is no other mode of explaining these facts, but by admitting that an inhibitory influence is exercised by the diseased trigeminus upon the centres.

It is not, however, by directly acting on nerve cells that those influences which cause what I call reflex paralysis and reflex contractions take place. I cannot conceive how such effects could well happen, for instance, in the cases reported by Weir Mitchell, of lesions of nerves of one inferior limb being followed by paralysis of one arm in the same side, etc. My belief is, that effects take place through the agency of the vaso-motor nerves. The diversity as to the seat of the reflex symptoms can be explained on the force of the law of "de locis minoris resistentiae," so ably taught by Prof. Verneuil, of Paris.

The vaso-motor nerves, I believe, do not take their origin in the spinal cord, nor do they have their origin in the pons, but run all along the spinal cord and middle encephalon, as stated by Vulpian, Setchenow and others. I think that there are experimental data enough to establish this theory, that the vaso-motor system has its centres in the ganglia along the spinal roots, and in disseminated ganglion cells found in the meshes of the pia mater of the cerebrum, and more particularly, around the pons and anterior portions of the hemispheres. At the level of the spinal ganglia the nerves send branches into the spinal cord along with the nerve roots, chiefly posterior, and out towards the periphery along the nerves terminating in different organs.

If this view is admitted, it becomes easy to reconcile the otherwise contradictory results obtained by means of the Wallerian method. This is more than an hypothesis with me, as I have lately said and written much about it, and stated on what experimental grounds I support it. The modes of production of these sensitive, sensory and motor palsies are of three kinds, as I have said, and as Brown-Sequard teaches, but properly speaking, they are all explained in the force of the vasomotor influence.

It has been urged that a reflex or other contraction of blood vessels cannot be of long duration, without consequent alteration of the tissues—of course there is something in that notion, but who has not observed cases of hysterical anesthesia? In these cases, as I have lately again satisfied myself, the anæsthesia depends upon contraction of the blood vessels of the integuments of the skin. If in a hysterical anæsthetic patient a pin or a very sharp and fine blade be passed to the depth of one millimetre into the skin, not only no pain is felt but no blood oozes. If a fold is made of the skin, and this well squeezed several times, then the same operation is followed by pain and by a little blood oozing; moreover, it has been ascertained by different authors, and I believe among others, by Dr. Charles Richet, that the irritation of nerve trunks does give rise to some degree of pain, whilst that of the nerve ends is without effect. In a word, we have to deal then with a case of loss of function through anæmia.

I contend that in cases of so-called reflex paralysis of sensation, as well as of aphasia and of amaurosis, the same condition obtains in the centres. The irritation in these cases instead of starting from the centres towards the periphery, on the contrary proceeds from the periphery towards the centres; so that if pain is not felt in cases of hemianæsthesia from destruction of the internal capsule, for instance, it is not because the impression is not transmitted from the periphery, but because at the place at which the impression is received ordinarily, or where the mandates of the will act, there is functional inability, due to alteration of the blood supply; this can be brought about in two ways, either the irritative influence in diseases of the bladder, the urethra, the kidneys, the intestines and other peripheric nerves, the lungs or the pleuræ, has been so strong that a state of paralytic dilatation of the blood vessel has taken place, identical with the one which follows irritation of the sciatic nerve, separated from its centre, and which has been said to be due to the action of dilating nerves (about which more hereafter), and the exchanges between the blood and the tissues are stopped, or the contractions of the blood vessels have so acted as to exhaust the receiving nerve cells—have brought on an inhibitory action. Only one set of nerves are concerned in these vaso-motor troubles—the vaso-constrictor. Claude Bernard, Schiff, and Goltz, and many others have introduced into science the idea that there is another set of nerves whose function it is to antagonize the action of the vaso-constrictor. But this is not the case. Vulpian has said, after Bernard, I believe, that when a vaso-motor nerve is irritated, if a dilatation ensues, it is because an action has taken place very similar to the phenomenon studied under the name of interference by physicists—that is to say, that two actions take place in the ganglionic nerve cells which are scattered in the blood vessels and which are thus paralyzed, being the victims of antagonizing forces. But more recent researches have proved that such is not the case, as it has been shown that the rapidity of the interruption of the current used serves to explain the phenomena. A rapidly interrupted current does bring on, after a *very* short period of contraction, a dilatation, but a current interrupted very slowly—one or two interruptions in a minute—does not bring on a dilatation. The cause is obvious. The nerve cells and nerve fibres require time for recuperation of their power through nutrition, and if they are made to work faster than they can recuperate lost energy they are paralyzed. This same theory explains what takes place in the centres—spinal cord and brain—in cases of reflex paralysis and contractions. In dogs I have laid bare the spinal cord and observed that irritation of one kidney with electricity does bring on a paraplegia. I have seen the blood vessels of the pia mater become invisible for some time, and then gradually enlarge until no more pulsation could be perceived in them, the blood appearing of a more pale color. The same experiment can be made on the ears of rabbits. The blood vessels relax to such an extent that the exchanges between the blood and the tissues do not take place at all, or not according to a normal standard.

It may be said that in such a case the paralysis might be due to the proliferation of new elements through the blood vessels — to diapedesis — to squeezing of the nerve tissues, by the consequent proliferation of connective tissues—on the strength of Cohnheim's theory. But if it is kept in mind that paraplegia in dogs and in rabbits takes place within an hour after the beginning of the experiment, and that no white corpuscle has ever been seen to come out completely through the sheath of a blood vessel within that time, and that they do not increase in their new media very rapidly, it becomes necessary to reject that theory, so far at least, as used to explain the paresis in the very beginning.

But so much for paralysis. Contractions more often recognize for their cause similar disorders in the membranes of the cord. It is often seen that the mere laying bare of an inch length area of the dorsal dura mater, will induce a tonic contraction of the whole posterior extremity of a dog, lasting for hours and for days. The cerebral dura mater can be experimented upon very easily, and these effects are readily observed.

The experiments made on the cortex cerebri go a good way to show reflex actions at work to inhibit speech or vision. It is seen that when one of the so-called psycho-motor centres is taken away, a paralysis ensues which lasts some days or weeks, but which ultimately disappears; just as readily does the same thing happen when some other area of the cortex is similarly irritated. The loss of the senses of sight, of hearing (very seldom), of smell, of speech, etc., are due to inhibitory influences, started by the lesions of the cortex, but not because the so called centres have been removed, as there is no relation whatever between the degree of the alteration of the cortex, its kind, and its seat on the one hand and the palsy, or the contraction or the loss of faculties observed on the other, as I have established elsewhere.

As to the treatment of these affections, I have relatively little to say, because the indication of therapeutic agents is determined by the nature of the disorder, or rather by a consideration of the physiological pathology.

First among the internal remedies is strychnia—but strychnia administered in such a dose as to obtain the full physiological action. Of course, it is only possible for me to make general statements on that subject of therapeutics, as so many circumstances need to be taken into consideration in every case. But when the paralysis is not due to inflammation, the remedy is good for all kinds of paralysis.

Arsenic, in cases of children chiefly, affected with contractions of that nature which seems to be associated more particularly with phimosis, and which are nothing but choreic, will give good results if pushed to high doses. Generally there is no need of the operation of circumcision, as lotions made of bromide of ammonium and water freely used answer the purpose; if the prepuce is adherent, of course it has to be detached, but I believe that circumcision in many cases does not relieve the patients, because the glans penis is exposed, its nerves are hyperæsthetic, and therefore it is a source of continual irritation.

In cases of paresis or of contractions, methodic movements very slowly executed will be of great service. For both, rest as recommended by Weir Mitchell will work well.

Among the iron preparations, I believe that none will be more useful than the protochloride, as it has been proved to be the state into which all iron preparations are reduced in the stomach; in anæmic cases it is very useful. Those cases in which there is hemianæsthesia and chorea, or aphasia, etc., derive no little benefit from the use of faradic electricity on the anæsthetic parts—*very* weak currents should be used and the metallic brush be applied as one electrode, the other being a wet sponge. Dry the parts well before using the brush. Sometimes it is necessary to use a rather strong current, but very seldom. It has been observed that a strong current will dull the sensation which was awakened by a mild one. The sensation returns very rapidly and the temperature of the parts rises as does the muscular power. But chiefly in cases of aphasia, amaurosis and hemichorea, the actual cautery applied to the nape of the neck is exceedingly useful, the instrument to be heated to whiteness and not allowed to burn the skin—to be used with a light hand and often repeated. When there are traces of inflammatory processes of the membranes or of the nervous tissues, blisters of mustard on the points of the spinal cord tender to the touch are very useful, and ergot internally, or belladonna, etc.

These are the general indications, which, I trust, are in accordance with what I believe is the pathology of the diseases considered, and which have, besides, when used empirically also afforded good results.

37 W. 33D ST., NEW YORK CITY.

ART. III.—THE PSYCHOLOGICAL PATHOLOGY OF PROGRESSIVE PARESIS.

BY EDWARD C. SPITZKA, M.D.

(*Read before the Neurological Society of New York City, Feb.* 5, 1877.)

IN the choice of progressive paresis, for my subject, I was determined, not so much by the intrinsic interest of this affection, as by the fact that through its pronounced and constant characters it furnishes the best illustration of certain general propositions, whose enunciation is my chief object this evening. I do not intend to give a detailed and continuous account of either the clinical symptoms or the morbid lesions of progressive paresis, but rather to exhibit the physiological relation existing between these two factors.